notoriety, Mr. Gray, whose instruments had also been called telephones, gave a public exhibition, in Chicago, I think, and in the report of his lecture which I read, he never once alluded to Bell's invention. His discourse was then, as at Philadelphia before the judges, solely in reference to the musical telephone. In fact, the newspapers had to take pains to inform the public that Mr. Gray's invention must not be confounded with Mr. Bell's, to which Sir William Thomson had referred. You will imagine, then, the surprise of the judges who examined these inventions particularly at Philadelphia in 1876, and heard the personal explanations made by the inventors, to be told now that Gray had already invented the speaking telephone, when all his statements then made show directly to the contrary.

Ann Arbor, November 18 JAMES C. WATSON

The Intra-Mercurial Planets

NATURE (vol. xviii. p. 569), in commenting upon my letter published the previous week, regarding the discovery of Vulcan, accused me of being not only "indefinite," but "contradictory." The number containing my letter (p. 539) has, from some unknown cause, not yet reached me, though I am in receipt of four numbers published later.

In the several articles written by me on that subject—to the Chicago Astronomical Society, to the Naval Observatory at Washington, to the Astronomer-Royal, to Admiral Muochez of the Paris Observatory, and to others—I have invariably stated the facts as they occurred under my observation, and as they impressed themselves upon my mind, and have invariably adhered to these statements, viz., that the two stars seen by me were of about the fifth magnitude, about 7′ or 8′ apart, with large red disks, and pointing towards the sun's centre. It is true my letter did contain an error, but not of observation, nor of estimation. In reducing the 8′ of arc (the estimated distance between the stars) to time, I somehow called it 2′, when, in reality, it is but 32s., thus not only changing its position in R.A., but also increasing, in this element, the discordance between Prof. Watson and myself. The detection of this error has changed, to me, the whole aspect of the Vulcan question. I had previously written to Prof. Watson that I could not reconcile his observations with my own either in R.A., or in Dec., but did not tell him what changes were necessary in order that they might harmonise. He gave me his corrected positions, which helped matters considerably, but still his R.A. was too great, and Dec. too little, for, from three estimations, the two stars ranged with the sun's centre. Recently I have been experimenting with α^1 and α^2 Capricorni (two stars which, in respect to distance from each other, resemble those I saw during the eclipse), my object being to test the accuracy of estimations made of the directions towards which two stars will range when hastily brought into the estimated centre of the field of a telescope having a diameter of one and a half degrees. I find that unless the objects are brought exactly to the centre, they do not point to the same place. During totality time was, of course, too precious to waste in being precise in this, and yet I endeavoured to be so, and as at each of the three comparisons they seemed to range with the sun's-centre, I feel convinced that I was not far out in my estimated Dec.

In order to meet Prof. Watson's excessive R.A., I published (contrary, however, to my better judgment), that the distance between the stars was about 8′ instead of 7′ (as previously announced). On the assumption, therefore, that (*a*) one of the objects was θ Cancri, and (*b*) that they were 8′ apart, and (*c*) that the one nearest the sun was the planet, as Watson says, the position of the planet was as follows:—

Washington M. T.

1878, July 29, 5h. 22 m. R.A., θ Cancri	...	8h. 24m. 40s.
Add 8′ =		32s.
Planet's R.A., Swift	...	8h. 25m. 12s.
,, ,, Watson	...	8h. 27m. 35s.
Difference	...	2m. 23s.
Dec. Swift	...	18° 30′
,, Watson ...	...	18° 16′
Difference	...	14′

It will be seen that there is a discrepancy between us of over a half degree of arc in R.A. If we saw the same objects how can we differ so widely? Could I be in error to the amount of 34′ between two stars in the same field? Can two stars be three and one half times the distance of Mizar from Alcor and an observer of experience estimate them at only 7′ or 8′? It will be remembered that I recorded in my note-book at the time the distance as 12′, but knowing how liable I might be to error in the valuation of so large a distance (for though, from practice, I can estimate quite closely double stars whose distances are from 2″ to 20″, I have had no experience in the estimation of those of several minutes separation), I chose to carry it in my mind until I should reach home, when it would be the work of only a few minutes to find two stars of the same apparent distance.

I said to Prof. Hough on our homeward journey, that, from memory, I thought their distance was about equal to that separating α^1 and α^2 Capricorni, and that I could decide when I should observe them. My memory of Mizar and Alcor was quite distinct, and as soon as I thought of those (which I did before my arrival at Kansas City) I mentally said, "A little over half the distance between them equals that between θ Cancri and the new object," which I did not doubt was Vulcan. Upon my arrival at home I immediately consulted "Webb's Celestial Objects," and was not a little surprised to find their whole distance to be less than 12′. Thus I know they were not over 8′ apart, I believe they were but 7′. I know they pointed to the sun's disk, I believe they did to his centre. I know they did not differ one-fourth of a magnitude in brightness, I believe they were exactly equal. I see them, in my mind's eye, as I then saw them, and, while consciousness endures, their image can never fade from the retina of my memory!

I consider the estimated distance in arc, made in such great haste, as valueless compared with the distance as impressed upon the mind from three comparisons, and verified by observations of a reliable character since arriving at home.

Can any error, then, be ascribed to the measurements of Watson, a skilful observer, with telescope well mounted, and with appliances for measuring, and who not only did measure the position of the new planet, but that of the sun and θ and δ Cancri (three objects in its immediate neigbourhood) as well.

Have we any right to call in question the accuracy of his circles in giving the position of the new object when they correctly gave the positions of the others?

Wherein, then, lies the discrepancy, and how can it be reconciled? Again, Watson says the planet was much brighter than θ, while the stars which I saw were of equal magnitude.

Several times since my return from the eclipse expedition I have, both in darkness and in strong twilight, examined θ, and I find no star near it, nor no two stars in its vicinity answering, in any particular, to those seen by me at Denver.

The above facts I submit to the world, and astronomers must deduce therefrom their own conclusions as to what the objects were. My own are reached, and, briefly stated, are as follows:—That the two objects seen by me were both intra-Mercurial planets, and that I did not—as was for a time supposed—see θ Cancri. Prof. Watson saw θ, and, some 42′ of arc south-east of it, another planet, and determined its position, and near to ζ Cancri still another, whose position also he fortunately ascertained, making four in all. It will not do to say, as some have intimated, that Watson saw θ Cancri, and 42′ from it a planet which I did not see, and that I, also, saw θ, and, 7′ or 8′ from it, another planet which he did not see. This reasoning appears to me untenable, for how could he have failed to see mine, when the diameter of his field was over 40′, and had θ in its centre?

If the above conclusions are true, and that four planets were discovered instead of two (as at first supposed), the question naturally arises, Which, if any one, is Lescarbault's Vulcan?

I estimated, at the time, the objects as being of the fifth magnitude, that is, as bright as a fifth magnitude star would appear in a clear, dark night. How much allowance ought to be made for diminution from atmospheric illumination I know not. I was then of the opinion that it would make a difference of at least one magnitude, but, having examined the region around θ, and finding many stars there, and several which are quite bright, not one of which I saw during the eclipse, I think that fully two magnitudes should be allowed.

In what way can these intra-Mercurial planets (of which there are probably many) be detected?

I would suggest that, on July 29 next, a determined and systematic effort be made, with large telescopes equatorially mounted, to observe θ Cancri, and, if then successful, there is hope that these planets, or some of the larger ones, may be discovered in the absence of a total eclipse, or while in transit. If

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θ cannot be thus seen, then it appears to me that all time spent in their search in the sun's vicinity, except during a total or very large partial eclipse, would be time lost.

Rochester, N.Y., November 8 LEWIS SWIFT

Colour-Variation in Lizards.—Corsican Herpetology

IN a communication sent to you by my friend Mr. Wallace, under the title, "Remarkable Local Colour-variation in Lizards," published in NATURE, vol. xix. p. 4, mention is made of the well-known case of *Lacerta* (Podarcis) *muralis*, var. *faraglionensis*, only found on the Outer Faraglione of Capri, but there are many similar cases to my knowledge, and I add a note of them, for the fact, although unexplained, is one of great interest. During the last two years, while engaged in forming a complete series of the Italian vertebrate animals, I have visited and explored most of the Mediterranean islands included in the Italian sub-region, and I have invariably found that our common lizard (*Podarcis muralis*) constantly presents dark varieties *on islets adjoining small islands:* this is the case on the Scuola, near Pianosa, on the Scoglio di Mezzogiorno, off Palmarola (Ponza), on S. Stefano, off Ventotene, on the Toro, off Vacca (Sardinia), on Lisca nera, Lisca bianca, and Bottaro, off Panaria (Lipari), on Filfla, off Malta, and on Linosa, near Lampedusa. The extreme cases are those of the Faraglione off Capri and Filfla, where a nearly intense black is obtained; next comes Toro, and next Linosa; only the latter case might be explained by the "struggle for existence" theory, for the lava rocks of Linosa *are* black; but such is certainly not the case with the other islets, and, *pace* Dr. Eimer, the Faraglione is gray, while Filfla—on which I spent a pleasant day in October last—is painfully white in the glaring Maltese sun, so that its black lizards are most conspicuous. I may add that few creatures I know vary more in colour than *Podarcis muralis*, even in the same locality; two most distinct varieties occur promiscuously on the small flat islet Formica di Grosseto.

Going over my Mediterranean herpetological notes reminds me of an interesting discovery I made last summer in Corsica, an island of great interest, which, strange to say, is rarely trodden by naturalists. Most of your zoological readers will be aware that, in 1839, Prof. Savi, of Pisa, described two new species of Italian Urodela, both from Corsica, viz., *Salamandra corsica* and *Megapterna montana*. The former has been quite neglected by modern herpetologists, or else placed among the synonyma of *S. maculosa*, simply because no one had Corsican specimens to compare. Now it is evidently nearly allied to the Continental form, but *quite distinct*, as the specimens I collected testify, all of them presenting the distinctive characters pointed out by Savi forty years ago. A nearly similar lot befell *Megapterna montana*, which Savi described nearly contemporaneously with Genè's description of *Euproctus Rusconii*, from Sardinia. Buonaparte, in his "Fauna Italica," united the two under the name of *Euproctus platycephalus*, given by Gravenhorst in 1829 to a newt, *sine patria*, preserved in the Breslau Museum; and most naturalists have followed Buonaparte, especially later writers on the subject, as Strauch, De Betta, and Schreiber, whilst others, acting more wisely, stuck to Genè's name. I believe that since Savi's day no one has studied the Corsican form, whose essential characters pointed out by the Pisan naturalist, who had only two specimens to work on, were overlooked even by his contemporary, the Prince of Canino; this explains all. Last year I rambled and collected all over Corsica, and found Savi's newt quite common in all the mountainous districts; I secured about 150 specimens of both sexes and all ages, even larvæ, and on my return to Florence was much surprised to find them *quite distinct* from the Sardinian *Euproctus* I possess; this made me refer to the original descriptions, and thus I found that Savi and Genè had described two very distinct species, and described them well. The two Italian species of *Euproctus* may be thus defined:—

E. Rusconii, Genè: Parotids wanting. Skin smooth, with small whitish tubercles scattered, especially about the sides of the head and neck. Female with a small conical pointed fibular tubercle, very like a rudimentary finger. Hind fingers slender and cylindrical. Irregular dark blotches on the throat. Size somewhat larger than the succeeding species. Hab. Sardinia.

E. montanus, Savi: Parotids small but distinct. Skin rough and granular. Female with a large, obtuse, compressed fibular tubercle, more like a ridge or crest, than anything else. Hind fingers stout, broad, and flattened. Throat uniform, rusty, without blotches; often a red or yellow dorsal stripe. Hab. Corsica.

As to *Euproctus platycephalus*, Gravenh., only a careful examination of the type-specimens, if yet existing in the Breslau Museum, can settle to which form it ought to be referred, but if their locality is unknown, I believe it better to suppress the name. *Euproctus platycephalus* is said to be found in Spain, but as I have no Spanish specimens, I cannot give any opinion on that form. In conclusion, I may add that Buonaparte was perfectly right in separating from the former the North African species *T. Poireti*, which is very distinct from our Italian *Euproctus*, in the shape of the head and body, and in the complete absence of any fibular tubercle in the female; it ought to be called *Glossoliga Poireti*. HENRY HILLYER GIGLIOLI

Florence, November 16

Commercial Crises and Sun-Spots

REFERRING to Prof. Stanley Jevons's article upon "Commercial Crises and Sun-spots" in NATURE, vol. xix. p. 33, I beg to draw your attention to the inclosed circular which I issued to my subscribers in April last.

The figures relating to the "Failures in England and Wales," were compiled by my clerks, under my own direction; those relating to the failures in the United States and Canada were supplied by Messrs. R. G. Dun and Co., of New York and London, and it may be observed how nearly they agree (*i.e.*, the failures in England and Wales, and those in the United States and Canada) in their fluctuations, and that there is an agreement between both sets of figures and the sun-spot period.

I have not been able to obtain similar figures for continental states, but I have observed that the complaints of depression in trade there agree, in substance and in time, with those in this country and North America. I have also noticed similar complaints from the southern hemisphere, especially New Zealand.

I refer to Dr. Hunter's suggestion of an Indian famine period in my circular, but I do not find that the famine period in India agrees, in point of time, with the depressions in the temperate zones; it is very probable that the excess of sunshine which produces drought and famine in India has an opposite effect on the prosperity of England and all other countries lying between the same isothermal lines, and that the more moderate degree of sunshine which may suit the Indian cultivator is insufficient to properly ripen English wheat and other produce (oats excepted).

Since April last I have taken several opportunities of ascertaining from agriculturists the effect of the variations in the sun-spots upon their yield of wheat, &c., and I find an agreement between them that during these years of minimum sun-spots the yield has proved bad when thrashed out, in consequence of the kernels being much smaller than in other years. I do not know whether the test has ever been tried or not; if not, I would suggest that some scientific observer should weigh an ounce, or a few ounces, of the kernels of each kind of grain grown in England every year, and count the number of them. I think it would be found that in years of maximum sun-spots wheat and barley kernels weigh their heaviest and oats their lightest, and that these proportions would be reversed in the years of minimum sun-spots. The difference in each kernel or in an ounce of them may, taken alone, appear trifling; but if it is an indication of the difference in the yield of the harvest throughout the whole kingdom, it may be a fact of the greatest importance as showing the cause of the cyclical variations in the prosperity of the country, and it may be of great value to land-owners and agriculturists generally as a guide in the rotation of crops and in allowing fields to lie fallow.

It is in this direction that I look for the causes of commercial depression. The whole of our "home" trade is dependent upon internal prosperity, and likewise a large proportion of our "foreign" trade. Other causes may have some effect upon either or both, such as peace or war, trade-unionism, bank-management, and the like; but the influence of the sun is too far-reaching and too powerful to be checked thereby. Man, by studying the working of its influence and power upon his daily life, may learn how to guard against much of the distress which periodically recurs. JOHN KEMP

Aspley Guise, November 16

"*London, April*, 1878

"*Failures in England and Wales*

"We append a Summary of the failures in England and

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